in conjunction with the negative or lower end of the needle. This is continued from high to low and from low to high water and from day to day, the result being recorded as read off. The mechanical difficulties in the construction of the machine are very great, but not considered insuperable.

## ON THE DEFICIENCIES OF METEOROLOGICAL WORK IN DATA OF VALUE TO AGRICULTURE, AND MEANS FOR SUPPLYING THEM.

By WILLIAM MCMURTRIE.

Meteorological records, as they are and have been and are being made, are deficient in many of those data which have the most important influence upon farm crops. Temperatures are recorded, but they are always observed in the shade. Rainfall is given, but often in such a way as to render its record of no value in the study of the development and condition of crops, because no indication is given as to the way in which it is distributed; light being of little importance to meteorologists generally, while it is one of the most potent factors in the development of vegetable and animal life, has been almost completely ignored. Late investigations have proven conclusively the importance of the tension of atmospheric electricity upon vegetation, and it should be regularly observed and recorded. In fact, meteorologists have principally confined themselves to the record and study of such conditions as enable them to predict the approach and occurrence of storms, thus looking more to the commercial than to the cultural side. Gasparin was the first to call attention to the importance of the relations of Meteorology to agriculture, and he has had at least two active followers—Quetelet in Belgium, and Marie Davy in France. Through the instrumentality of the latter there has been established, near Paris, an observatory of Agricultural Meteorology, where observation and record of all the conditions above named is made. The results already obtained have shown great practical value, and worthy of the means and labor required in securing them. In this country we have nothing similar to it. Our Signal Bureau, as nearly perfect as may be for the purposes for which it was designed, is devoted to the record and study of those observations as will render possible the prediction of future conditions which may affect human affairs, than such as may influence the development of crops. Besides this, the number of stations at which observations are made in this country is too limited, being not over 800, while for agricultural work 3,000 would not be excessive. Additional work should, therefore, be carried on, and observations at a larger number of stations made and recorded, to be discussed in connection with the records of observations made upon the condition of the crops. The nature of the work is such that it should be undertaken by the Department of Agriculture, and the organization of the latter with the 2,300 reporters it already employed would be well adapted to it. Fortunately, General Le Duc, the Commissioner of agriculture, is in favor of the establishment of such work in the Department, but will require congressional support to enable him to do so. The plan of work suggested by the author is as follows: 1. The establishment of a system of observation and record among the reporters of the Department of Agriculture, and others whose co-operation could be secured throughout the United States and Territories, with instructions to observers to keep careful records of the conditions of atmospheric pressure, temperature in its various relations, relative humidity, evaporation of moisture, winds, light, tension of atmospheric electricity, occurrence of dews, fogs and frosts, and report them at stated intervals of time to the Department for consideration and permanent record. I. The collection of meteorological records from every part of the world, from which to construct detailed tables showing the relations of all the conditions named above, and may influence the growth and health of vegetation. 3. The construction of maps showing the geographical distribution of crops, to be used in connection with the meteorological or climatic data to be collected.

## PRELIMINARY ACCOUNT OF A SPECULATIVE AND PRACTICAL SEARCH FOR A TRANS-NEPTUNIAN PLANET.

By D. P. TODD, M. A., Assistant in the Office of the American Ephemeris and Nautical Almanac.

So early as the year 1834, HANSEN was credited with expression of the opinion, in correspondence with the elder BOUVARD, that a single exterior planet would not account for the differences between the tabular and observed longitudes of the planet Uranus. Dr. GOULD, however, in his "Report on the History of the Discoveryof Neptune," says: "I have the authority of that eminent astromener himself (HANSEN) for stating, that the assertion must have been founded on some misapprehension, as he is confident of never having expressed or entertained that belief."

Professor PEIRCE'S criticism of the investigations of LE VERRIER, to the effect that his predicted orbit of Neptune was so widely discordant from its observed orbit as to indicate that his computations did not pertain to the actual disturbing planet, elicited from him the reply that the perturbations of Uranus due to a possible planet exterior to Neptune might readily cause an uncertainty of 5″ to 7″ in the fundamental data of his research.

In 1866, the Smithsonian Institution published the general tables of Neptune, by Professor Newcomb. In the investigation of its orbit the author proposed: "3. To inquire whether those motions [of Neptune] indicate the action of an extra-Neptunian planet, or throw any light on the question of the existence of such a planet." He concludes (page 73) that it is "almost vain to hope for the detection of an extra-Neptunian planet from the motions of Neptune before the close of the present century."

In 1873, the Smithsonian Institution published the general tables of Uranus, by Professor Newcomb. His success in the treatment of the theory of Uranus was such that astronomers generally may be said to have been satisfied from the smallness of the longitude-residuals, that there existed no evidence of perturbative action upon Uranus other than that actually taken into account in the construction of the tables. It is well known, however, that since the publication of these tables the error of longitude has been increasing.

Sometime in the spring of 1874, the first preliminary outline of the very simple method which I have here employed in the treatment of planetary residuals with reference to exterior perturbation, suggested itself to me. For more than three years very little opportunity offered for consideration of the problem of a trans-Neptunian planet. In August, 1877, however, I began to devote the larger portion of my leisure time to the theoretic side of the question. It was soon evident that no certain hold upon any possible cause of exterior perturbation could be obtained from the residuals of Newcomb's tables. And I may remark here that I have consequently chosen the term *speculative* rather than *theoretic* as applying more fitly to the investigation which preceded the actual telescopic search.

It did not seem to me that the magnificent researches of Le Verrier and Adams on the perturbations of Uranus should be taken as models in the present investigations, for two reasons:

(1) The residuals of longitude which must form the basis of the investigation are not sufficiently well marked to justify the execution of so laborious a research, especially if it be found that a simple, rational treatment, unencumbered with the refinements of analysis, may be fairly interpreted as indicating the position of an exterior perturbing body with merely a rough approximation.

(2) Even in the case of Uranus, and the theoretic search for Neptune, where the residuals of longitude were very strongly marked, many of the elements pertaining to the disturbing planet, which Adams and Le Verrier sought to determine theoretically, turned out afterward, when their real values became known, to have been indicated with only meagre precision. Much less should we now expect these elements to be given with any certainty in the case of a planet exterior to Venus.

This provisional treatment of the residuals of Uranus was undertaken, then, as a preliminary to the proposed

telescopic search to determine whether that search was worth undertaking, and if so, at what point approximately it was best to begin.

I.—We now consider, *seriatim*, the errors of the elements of the perturbed planet—errors which the very hypothesis of a disturbing body introduces, and which must have entered into the tables of the inferior planet, as constructed independently of unknown exterior perturbation. We consider what the effect of these errors may be, and how far it may be eliminated or subtracted from the residuals of the actual theory of the planet. These residuals are, of course, first corrected for any known error of theory or tables, or erroneous masses of known perturbing planets.

(1) *The error of mean distance of the perturbed planet.*—Any error of radius vector enters very largely into the residuals of heliocentric longitude, if the observations are made at any considerable intervals from the planet's opposition. If it is suspected that the error of radius vector will vitiate the residuals of longitude, we may avoid its effect by passing to residuals of geocentric longitude. Or we may confine our research to the mean residuals of observations near the opposition points, and symmetrically placed with reference thereto. The effect of erroneous radius vector is thereby eliminated.

(2.) *The error of periodic time of the perturbed planet.*—If the residuals are examined graphically, the eye will readily detect whether any correction to the periodic time is advisable. If, in general, the mean line of the residuals is nearly a right line, and makes a given angle with the line of zero-residual, it may fairly be concluded that the residuals need a correction depending directly on the time, the magnitude of the co-efficient of which is indicated by the divergence of the two residual-lines.

I had considered the problem only thus far when it occurred to me to apply the method, only partially developed, to the determination of an approximate position of Neptune from the residuals of Bouvard's Tables of Uranus, published in 1821. Taking also the residuals from observations up to 1824, and not permitting myself a knowledge of the longitude of Neptune at any epoch, a very little labor gave me an approximate position of the disturbing planet from which, it now appears, Neptune might easily have been found some twenty years in advance of its actual discovery.

When my work had advanced to this stage, a mere chance threw in my way a copy of Sir John Herschel's *Outlines of Astronomy*, (which I had never before examined): I at once observed that my treatment of the residuals of Uranus with reference to a planet exterior to Neptune was quite similar to his "dynamical" exposition of the perturbations of Uranus arising from Neptune itself. And I was farther gratified to find that he had given a very full and lucid statement of the effect upon the longitude-residuals caused by errors of the third and forth elements of the perturbed planet—the error of eccentricity, and the error of longitude of perihelion.

(3.) *The error of eccentricity of the perturbed planet.*—(See Sir John Herschel's *Outlines of Astronomy*, page 536.

(4.) *The error of longitude of perihelion of the perturbed planet.*—(Ibid., page 537.)

When the longitude-residuals have been corrected in this manner, we proceed on the assumption that any outstanding residuals are due to unexplained exterior perturbation.

II.—Of the seven elements of the disturbing planet, we must assume a value of one: the values of three others, together with the mass of the disturbing planet, we may consider as theoretically determinable from the longitude-residuals themselves.

(1.) *The mean distance of the disturbing planet.*—Regarding the next order of distance beyond Neptune as occupied by the planet for which we are searching, I assumed, as a first value of mean distance, $a=46.0$: this value seemed to be indicated by a fair induction. The periodic time of the planet would then be 312 years, and conjunctions with Uranus would occur nearly at intervals of 115 years.

(2.) *The eccentricity of the disturbing planet.*—Even with the large residuals of Uranus employed in the investigations of Le Verrier and Adams, the derived value of the eccentricity of Neptune was entirely illusory. The several values of eccentricity of Neptune resulting from their investigations are as follows:

| | |
|---|---|
| Adams (*first hypothesis*) | 0.16103 |
| Le Verrier | 0.10761 |
| Adams (*second hypothesis*) | 0.120615 |

The eccentricity given by investigation of the orbit of Neptune from observations of the planet was:

| | |
|---|---|
| Newcomb (*Tables of Neptune*) | 0.0089903 |

We should, therefore, expect nothing of any attempt to arrive at the eccentricity of an orbit exterior to that of Neptune.

(3.) *The longitude of perihelion of the disturbing planet.*—Much the same remark obtains in reference to this element. The several values of longitude of perihelion of Neptune, resulting from the researches in perturbations of Uranus, are as follows:

| | |
|---|---|
| Adams (*first hypothesis*) | 315° 57′ |
| Le Verrier | 284° 45′ |
| Adams (*second hypothesis*) | 299° 11′ |

The longitude of perihelion given by observations of the planet is:

| | |
|---|---|
| Newcomb (*Tables of Neptune*) | 46° 6′ 39″ .7 |

Evidently it would not be wise to include this element in the investigation.

(4.) *The epoch of the disturbing planet.*—If we can obtain even a rough approximation to the value of this element, the end of the investigation is fully attained. An inspection of the outstanding residuals, graphically exhibited, will show, without further labor, the epochs of maximum disturbance. We may prepare an approximate perturbative curve, the epochs of maximum disturbance of which shall be in harmony with the assumption of mean distance of the exterior planet. By applying this to the plot of outstanding residuals, we may decide at what points the application of the perturbative curve best accounts for them. The amount of excursion in its several sinuses we need not, for this purpose, attend to with any great care: this will depend upon the mass and distance of the disturbing planet; and, that it will be unavailing to attempt any determination of the mass in the present case will be evident from the fact that the mass of Neptune, from the theoretical investigations of LeVerrier and Adams, was widely discrepant:

| | | |
|---|---|---|
| Adams (*first hypothesis*) | 0.0001656 | $\frac{1}{6039}$ |
| Le Verrier | 0.0001075 | $\frac{1}{9300}$ |
| Adams (*second hypothesis*) | 0.00015003 | $\frac{1}{6666}$ |

While the most reliable mass of Neptune from observation was:

| | | |
|---|---|---|
| Newcomb (*motion of the satellite*) | 0.00005160 | $\frac{1}{19380}$ |

We thus have the inverse problem of perturbation reduced to a very simple rational form. The residuals of longitude of Uranus were next treated in accordance with this method.

In his *Investigation of the Orbit of Uranus*, Newcomb presents three series of residuals; the mass of Neptune finally adopted in the tables, $\frac{1}{19700}$, corresponds very nearly to the mean of the first and third series. But the mass of Neptune which was employed in this investigation is that given by Newcomb's discussion of the motion of the satellite of Neptune, and is $\frac{1}{19380}$. Our first step, then, was to correct these mean residuals into accordance with this adopted mass.

Afterward, examining these corrected residuals according to the method just related, in reference to unexplained perturbing action, I concluded that Uranus was in conjunction with an exterior perturbing body between the years 1780 and 1795, and that another conjunction would take place at some time before the close of the present century. The most probable position of the exterior planet I therefore considered to be about 170° of longitude; the probable error of the position I considered roughly 10°. This result was reached on the morning of the 10th of October, 1877. During the few days immediately following I reviewed this examination, as much as possible independently of the previous result, and at the same time varying the assumed mean distance. With a value of $a = 52.0$ (which I finally considered inductively the most probable) I set down the longitude of the exterior planet equal to $162° \pm 6°$. This result was reached on the evening of the 14th of October. I now turned my attention toward a similar treatment of the residuals of Neptune, with a slight hope of getting a confirmatory result. Two suppositions agreed in fixing the longitude at about 180° to 200°, respectively. I therefore, on some day in the latter part of

October, 1877, wrote down as the exposition of all my inquiry the following results:

EXTERIOR PLANET.—Longitude (1877.84), 170° ± 10°.
Mean distance from the sun, 52.0.
Period of revolution about the sun, 375 years.
Mean and daily motion, 9″.46.
Angular diameter, 2″.1.
Stellar magnitude, 13+.
Longitude of ascending node, 103°.
Inclination of orbit to ecliptic, 1° 24′.

If a new disturbing planet exists in the longitude here indicated, nearly a century must elapse before its existence can be asserted at all positively from the residuals of Neptune alone.

I should never have been able to execute the telescopic search consequent upon the investigation just related, had it not been for the courteous offices of Rear Admiral Rogers, Superintendent of the Naval Observatory, and Professor Hall, in charge of the great refractor. It was with this instrument—the 26-inch equatorial—that the search was conducted. It seemed to me that I should begin the search at a point about 20° preceding that indicated as the most probable position of the planet, and continue it to a point following by the same distance. But, a careful search extending over a zone of this length, and of sufficient width to be certain to contain the supposable planet, would be a work of such magnitude that I could not expect its completion under several years. I therefore had recourse to an inductive determination of the inclination and longitude of node of the planet's orbit.

I computed anew the position of the invariable plane of the solar system. A differential comparison of its inclination with the inclination of the orbits of the major planets, gave, with little uncertainty so far as the mere induction was concerned, the inclination of the orbit of the trans-Neptunian planet equal to 1° 24′. Similarly I obtained for the longitude of node, though not so certainly, 103°. For the preliminary search I determined to fix the latitude-limits of the zone at a width of one degree to the north and one degree to the south of this adopted plane. To these elements I strictly adhered, with the intention, however, of alternately increasing and decreasing the inclination, and varying the longitude of node if I should arrive at no successful result from the search of this limited zone.

I may remark that the detailed plan of the instrumental search had been completely digested and written out as early as the 5th of September. To assist in a decision as to what method of search I should employ, I had recourse to an inductive consideration of the real diameters of the known planets of the solar system. I arrived at the result that a diameter of 50,000 miles might be taken as the minimum value for a planet next beyond Neptune. On this assumption, the mean distance of 52·0 gave for its apparent diameter 2″.1. I did not, therefore, hesitate in adopting the method of search depending upon the detection of the planet by contrast of its disk and light with the appearance of an average star of about the thirteenth magnitude. In the actual search, a power of 600 was often employed, but most of the search was conducted with a power of 400 diameters.

On thirty clear, moonless nights, between the 3d of November, 1877, and the 5th of March, 1878, this search was carried on after the manner I have indicated.

After the first few nights I was surprised at the readiness with which my eye detected any variation from the average appearance of a star of a given faint magnitude: as a consequence whereof my observing book contains a large stock of memoranda of suspected objects. My general plan with these was to observe with a sufficient degree of accuracy all suspected objects. On the succeeding night of observation these objects were re-observed; and, at an interval of several weeks thereafter this observation was again verified. At 3 A. M., the 6th of March, 1878, the search was discontinued—my observing book ends with the following note:

"The adopted plane of orbit of trans-neptunian planet is now searched (without break) from

$$v=146.8°$$
to $v=186.1.°$"

I have much confidence in this telescopic search—my aim was to sweep the zone so carefully that there should be no pressing need of duplicating it.

I ought not to conclude this paper without adverting to the apparently long delay of its publication. From the very beginning I had approached the entire problem of search for a trans-neptunian planet with resolute direction toward the end which I regarded of the highest scientific import—that of *finding the possible planet at the earliest moment;* if I were successful, observations of its position would then be secured at once, and an accurate determination of its elements would be a matter of earlier realization—it seeming improbable that any prior chance observation would ever be brought to light. After pursuing the theoretic side of the question for a short time, I saw clearly that many years must elapse before the perturbing action of this body on any interior planet would afford anything like pronounced evidence of its existence; recourse must be had to the practical telescopic search. So I tarried longer with the residuals of Uranus only in the hope of a possible shortening of the search by some indication that the planet was more probably in one portion of the heavens than in another. After the telescopic search, which I was conducting, had been temporarily brought to an end, by circumstances beyond my control, I was not without hope of effecting some arrangement whereby I might resume the search at an early day, and carry it to a satisfactory conclusion. After much thought upon the apathetic reception with which the magnificent researches of Adams and LeVerrier had met, I reached the conclusion that no competent observer would be led to continue the search through knowledge of the little work of speculation that I had done. And, as the work was undertaken with the end always in view of finding the planet, it did not appear that any advantage would result from its publication.

It will be remarked that this matter now assumes a very different aspect: the publication of a recent memoir *On Comets and Ultra-Neptunian Planets*, by Professor George Forbes, of Glasgow, assigns, by a method of investigation entirely independent of my own, a position to a possible trans-neptunian planet which may be regarded as in exact coincidence with that which I have deduced. The assumption of a mean distance 100, indicated in Professor Forbes' paper, will not appreciably destroy the representation of the residuals with which I have dealt. I have not yet been able to convince myself that the remarkable harmony of the results of the two investigations is simply a chance agreement; and, with the hope that the accumulated evidence of the existence of a far exterior planet may not fail to incite some observer in possession of sufficiently powerful telescopic means to a vigorous prosecution of the search, I have prepared this preliminary paper in order that attention may be called to the matter in sufficient advance of the opposition-time now approaching. I may add here, that, should a careful and protracted search of the region adjacent to the indicated longitude prove unavailing, no more certain test of the existence of a trans-neptunian planet admits of application within the next few years than that of telescopic search of a limited zone extending entirely around the heavens—a search which I have been hoping, for more than two years past, for an opportunity to undertake, but which I see no present prospect of realizing.

NAUTICAL ALMANAC OFFICE, Washington, August 4, 1880.

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IN the province of Keen-chang, China, $15,000,000 worth of a peculiar vegetable wax is annually produced. It is formed on the twigs of an evergreen tree (Ligustrum lucidum), whose oval leaves furnish homes for myriads of insects. These, during the spring, produce a thin skin over the leaves, from which exudes a waxy substance that hardens in the month of August. The twigs are then cut and boiled in water, by which means the purified wax is easily separated.